

the best naturalists had speculated on the probability of evolution, he for the first time found, as he innocently enough observes, that evolution and natural selection are not quite the same thing. Having made this highly original discovery, he forthwith proceeds to display a feebleness of judgment even more lamentable than his previous ignorance. For he concludes that the older speculations on the causes of evolution are more satisfactory than those advanced by Mr. Darwin. In the columns of a scientific journal any comment on such a conclusion might well be deemed superfluous, although Mr. Wallace, in his review above mentioned, had the courtesy to expose its folly. The older evolutionists deserve indeed all honour for having perceived early in the day that some theory of descent must be true, even though they were not able to find the theory that could be seen to be in any measure satisfactory. But a man who in the full light of Darwin's theory can deliberately return to "the weak and beggarly elements" of Lamarck—such a man only shows that in judgment he is still a child. The extreme weakness of Mr. Butler's argumentation has, as we have said, already been shown by Mr. Wallace; but it is of more interest to ask what infatuation it can have been that led him to suppose "all Europe and those most capable of judging" required him as an author to make himself ridiculous as an expounder of this subject. The answer is not far to seek. As Mr. Butler himself has told us, he has vanity, and his vanity is not less childish than his judgment. Thus, to give only one illustration. Of so much importance does he deem his own cogitations, that in the book we are reviewing he devotes two chapters, or more than thirty pages, to "How I wrote 'Life and Habit,'" and "How I wrote Evolution, Old and New"; entering into a minute history of the whole course of his speculative floundering. This is the only part of the book that repays perusal; but that this part well repays perusal may be judged from the following, which we present as a sample:—

"The first passage in 'Life and Habit' which I can date with certainty is one on p. 52, which ran as follows: . . . 'Do this, this, this, which we too have done, and found our profit in it,' cry the souls of his forefathers within him. Faint are the far ones, coming and going as the sound of bells wafted on to a high mountain; loud and clear are the near ones, urgent as an alarm of fire.' This was written a few days after my arrival in Canada, June 1874. I was on Montreal Mountain for the first time, and was struck with its extreme beauty. . . . Sitting down for a while, I began making notes for 'Life and Habit,' of which I was then continually thinking, and had written the first few lines of the above, when the bells of Notre Dame in Montreal began to ring, and their sound was carried to and fro in a remarkably beautiful manner. I took advantage of the incident to insert them and there the last lines of the piece just quoted. I kept the whole passage with hardly any alteration, and am thus able to date it accurately. . . . Early in 1876 I began putting these notes into more coherent form. I did this in thirty pages of closely-written matter, of which a pressed copy remains in my commonplace-book. I find two dates among them—the first 'Sunday, February 6, 1876'; and the second, at the end of the notes, 'February 12, 1876.'

This historical sketch, which is without the smallest interest to any one but Mr. Butler himself, winds up with the following burst of eloquence:—

"Here, then, I take leave of this matter for the present.

If it appears that I have used language such as is rarely seen in controversy, let the reader remember that the occasion is, so far as I know, unparalleled for the cynicism and audacity with which the wrong complained of was committed and persisted in. I trust, however, that, though not indifferent to this, my indignation has been mainly roused, as when I wrote 'Evolution, Old and New,' before Mr. Darwin had given me personal ground of complaint against him, by the wrongs he has inflicted on dead men, on whose behalf I now fight, as I trust that some one—whom I thank by anticipation—may one day fight on mine."

Mighty champion of the mighty dead! When our children's children shall read in Westminster Abbey the inscription on the tomb of Mr. Samuel Butler, how will it be with a sigh that in their day and generation the world knows nothing of its greatest men! But as it is our misfortune to live before the battle over Mr. Samuel Butler's memory has been fought, we respond to his abounding presumption by recommending him, whatever degree of failure he may have experienced in art, once more to "consider" himself "by profession a painter"—or, if the painters will not have him, to make some third attempt, say among the homœopathists, whose journal alone, so far as we are aware, has received with favour his latest work.

GEORGE J. ROMANES

NEWTON'S BRITISH BIRDS

A History of British Birds. By the late William Yarrell, V.P.L.S., F.Z.S. Fourth Edition, revised by Alfred Newton, M.A., F.R.S. Part 10, November, 1876; 11, September, 1877; 12, October, 1878; 13, June, 1880. (London: Van Voorst.)

WE call this work advisedly "Newton's British Birds," although the title-page would seem to signify that it is only a fourth edition of Yarrell's well-known "History." It is however in fact a new book. The text has been completely rewritten, and the familiar woodcuts and vignettes alone remain to remind one of the former author.

The parts of Prof. Newton's work now before us conclude the account of the Passeres and contain the commencement of the history of the British Picariae. We need hardly say that the article upon each species is worked out in the same careful and accurate way as in the former portion of this work. Prof. Newton, as every ornithologist knows, is our leading authority on this subject, which, during a course of many years of constant attention, he has made specially his own. We observe with great pleasure the elaborate manner in which the distribution of each species is described, not only within the area of the British Islands, but also wherever it is known to occur on other parts of the world's surface. We may likewise notice the entire absence of misprints and the excellence of the type and paper, which do credit alike to the author and publisher, and will no doubt greatly contribute to extend the circulation of the work. Having said thus much, it is with regret that we must add one word of discontent, for which we trust Mr. Van Voorst and Prof. Newton will alike forgive us. The rate of issue of the numbers is so slow that it is difficult to calculate when the new edition will be completed. As will be seen by the heading of the article, only four parts

have been published during the four past years. If, as we suppose, about twenty more parts are required to finish the work, it is manifest that unless the present rate of progress be expedited it will be twenty years before we are able to send our new "History of British Birds" to the binders. The edition was commenced, we believe, in 1871. Now thirty years seems rather long for the execution of a new edition of any work, even with all the improvements which, as we have shown above, the present editor has doubtless bestowed upon it. We would fain ask therefore whether the author and publisher cannot manage to move on a little faster. If this cannot be done it appears to us that the first portion of the work will be almost out of date before the last part is published, and that the subscribers will have good reason to complain.

OUR BOOK SHELF

Jahrbücher für wissenschaftliche Botanik. Herausgegeben von Dr. N. Pringsheim. Elfter Band, drittes und viertes Heft. With twenty-four plates. (Leipzig: W. Engelmann, 1877 and 1878.)

DR. JAKOB ERIKSSON describes in a lengthened paper the protomeristem of the roots of Dicotyledons, and directs attention to the four great types of structure observable in these roots. In the first type the apex consists of three separate zones of meristem: the plerome, periblem, and dermocalyptrogen. In the second type only two zones are present: the plerome and a common zone for primary cortex, epidermis, and root-cap. In the third type there is a common meristem zone from which all the others develop; while in the fourth there are two zones, the periblem and the plerome. Two additional types are met with in Monocotyledons: (1) in which there are four zones of meristem: calyptrogen, dermatogen, periblem, and plerome; and (2) in which there are three zones: the calyptrogen, the plerome, and a common zone for cortex and epidermis.

The germination of *Equisetum* and *Schizaeaceæ* forms the subject of two papers, one by Sadebeck and the other by Bauke, whose work was arrested by premature death. Woronin contributes a paper on the *Plasmodiophora Brassicæ*, the remarkable Myxomycete which seems to be the cause of the so-called Hernia of the cabbage plant, which has recently attracted so much attention.

The remaining papers are by Reinke, on *Monostroma bullosum* and *Tetraspora lubrica*. Wydler discusses at great length the morphology of certain forms of inflorescence, chiefly dichotomous; and lastly there is a paper by Pitra on the pressure in stems during the appearance of bleeding in plants. The contents of the parts are, as will be seen, very varied and deal with many different departments of botany, and will be found to sustain the reputation of the "Jahrbücher" so long associated with the name of Pringsheim.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of rejected manuscripts. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Unconscious Memory—Mr. Samuel Butler

WILL you kindly allow me a portion of your valuable space in order that I may demonstrate the completely groundless character of a series of insinuations which Mr. Samuel Butler

has made not only against myself, but also against Mr. Charles Darwin, in the work which he has recently published, entitled "Unconscious Memory" (Op. 5).

1. Mr. Butler insinuates that Mr. Darwin caused my essay on Dr. Erasmus Darwin to be translated simply in order to throw discredit on his work, "Evolution, Old and New" (Op. 4), which was published in May, 1879. Upon this point I have to observe that Mr. Darwin informed me of his desire to have my essay published in English more than two months before the appearance of Mr. Butler's book; that the translation did not appear earlier is due to the fact that I asked for a delay in order that I might be able to revise it.

2. The assumption of Mr. Butler that Mr. Darwin had urged me to insert an uncharitable attack upon him (Mr. Butler) in my sketch, is not only absolutely unfounded, but, on the contrary, I have to state that Mr. Darwin specially solicited me to take no notice whatever of Mr. Butler's book, which had in the meantime appeared. Since however I thought it desirable to point out that Dr. Erasmus Darwin's views concerning the evolution of animated Nature still satisfy certain thinkers, even in our own day (a fact which must add greatly to Dr. Darwin's reputation), I have made some remarks upon the subject in a concluding paragraph, without however naming Mr. Butler. And I may here emphatically assert, that although Mr. Darwin recommended me to omit one or two passages from my work, he neither made nor suggested additions of any kind.

3. Mr. Butler's assertion that the revision of my translation was made "by the light" of his book is only in so far justifiable that I looked over the latter before sending off my work, and that my attention was thereby called to a remark of Buffon's. From Mr. Butler's book I have neither taken nor was I able to take the slightest information that was new to me concerning Dr. Erasmus Darwin's scientific work and views, since in it practically only one portion of the "Zoonomia" is discussed at any length, and this portion I had already quoted and analysed, while Mr. Butler only refers to one comparatively unimportant part of the "Botanic Garden," and absolutely ignores the "Phytologia" and the "Temple of Nature." So that no single line of Mr. Butler's far from profound work was of the slightest use to me.

Mr. Butler's contention that I have quoted from his book a remark from Coleridge is entirely without foundation. I have been acquainted with this remark for years, and from the source quoted. It is also quoted in Zockeler's work (vol. ii. p. 256), mentioned by me on p. 151, which appeared prior to Mr. Butler's book (Op. 4). The whole of my indebtedness to Mr. Butler reduces itself therefore to a single quotation from Buffon.

4. Finally, as concerns the main accusation that no mention is made in the preface of the fact that my essay had been revised previously to publication, it is clear, as even a child could not fail to see, that this is not due to design, but is simply the result of an oversight. It would be simply absurd for a writer intentionally to attack a publication which appeared subsequently to the date indicated on his title-page; and the so-called falsification, so far from injuring Mr. Butler, could only be most agreeable to him, because it might induce the careless reader to fancy that no reference whatever was intended to Mr. Butler in the closing sentence. Should however such a reference be clearly intended—and to every reader posted up in the subject this could not be doubtful—every man of common sense would recognise this terrible falsehood to be a simple oversight.

Beijing, January 12

ERNST KRAUSE

Hot Ice

I VENTURE, in referring to Dr. Lodge's letter of this week, to put before your readers the meaning of the remarks made on Dr. Carnelley's experiment at the Chemical Society by Prof. Ayrton, who is now away from England. I understood him to say that as Dr. Carnelley's hot ice is obviously in a condition which cannot be represented within the as yet known fundamental water surfaces, it is necessary to produce these surfaces beyond the places at which, hitherto, abrupt changes have been supposed to take place in them. He took as an instance the ice-water surface which has hitherto been assumed to stop at Prof. James Thomson's "triple point," and showed that although Sir Wm. Thomson's experiments have proved that it is nearly plane for the stable state of water and ice, yet in the imaginary district beyond the triple point a change of latent heat might give such a change of curvature as to bring this surface into the hot-ice region.